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AUTHOR Summers, Anita A.; Wolfe, Barbara L.
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ABSTRACT

On the basis of a microeconomic examination of Philadelphia School District data, it is concluded that (1) many school inputs do matter, (2) disadvantaged students can be helped by particular types of inputs, and (3) these optimistic results are accounted for by the use of pupil-specific data, and appropriate statistical data. The school inputs examined include teacher quality and school quality (e.g., class size). Peer group effects are discussed. Genetic and socioeconomic inputs of the students are examined. The importance of examining school inputs differentially is established by a comparison of the results with and without interaction analysis. The importance of using disaggregated data is emphasized by a comparison of the results when pupil-specific data for school inputs are used with the results when school averages are used. (Author/MC)

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DO SCHOOLS MAKE A DIFFERENCE?

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Do Schools Make a Difference?

By ANITA A. SUMMERS AND BARBARA L. WOLFE*

Parents, courts, and legislatures have been struggling to define equal educational opportunity (minimum achievement level for all? minimum growth in achievement? differential growths in achievements?). At the same time, economists, sociologists, and educators have been struggling to identify which package of school inputs is required for each type of student to equip him or her for educational growth. Most empirical attempts to identify which inputs matter have concluded that schools barely make a difference. From this conclusion has flowed a prevailing nihilism with respect to schools as an egalitarian force. We conclude, on the basis of a microeconomic examination of Philadelphia School District data, 1) that many school inputs do matter, 2) that disadvantaged students can be helped by particular types of inputs, and 3) that the use of pupil-specific data, and statistical methods appropriate to such data, account for the cheerier results of this study.

Little theory, economic or otherwise, is currently available to describe the determinants of educational achievement. Casual observation, combined with the education literature, suggests that achievement (A) is a function of a student's hard-to-disentangle genetic endowment and socioeconomic status ($GSES$), teacher quality (TQ), non-teacher school quality (SQ), and peer group characteristics (PG). Thus,

$$(1) \quad \Delta A = F(GSES, TQ, SQ, PG)$$

This equation is, of course, a reduced form. Attitudinal variables may affect achieve-

ment, but are themselves determined by the four factors listed above.

Much of the empirical literature that has arisen in the years following the Coleman Report (see James S. Coleman et al.) has focussed on estimating equation (1). Many investigators have viewed it as a production function relationship, describing educational output as a function of inputs. In education, all inputs cannot be selected as in a factory. Such a view, therefore, blurs the distinction between the variables which the educational policymaker can control (TQ , SQ , some PG) and those which he cannot ($GSES$, some PG). Further, the production function, as used in its classical context, relates the maximum attainable level of output for given inputs to the level of inputs—it describes the boundary of the production set. There is little reason to believe that we now know enough to have any confidence at all that schools are attaining such productive efficiency. In any case, it is clear that estimation procedures based upon cost-minimization assumptions are inappropriate. It seems preferable, therefore, to view (1) as a simple input-output relationship.

Past attempts at estimating (1) have represented many inputs by school- or district-wide averages, rather than by the more appropriate pupil-specific data;¹ we use pupil-specific proxies for the variables and find that much can be revealed by disag-

*Research officer and economist, Federal Reserve Bank of Philadelphia; consultant, Federal Reserve Bank of Philadelphia and economist, Institute for Research on Poverty, University of Wisconsin, respectively. We are indebted to Robert Summers and Lawrence H. Summers for their suggestions and interest, and to the School District of Philadelphia for their willingness to allow us access to their detailed data.

¹School inputs were measured at the district level in the studies by Charles Benson et al., Elchanan Cohn, and Herbert J. Kiesling; they were measured at the school level in the studies by Harvey A. Averch and Kiesling, Coleman, Jesse Burkhead et al., and Martin T. Katzman; the studies by Samuel Bowles, Eric Hanushek, Henry M. Levin, and Stephan Michelson, which drew upon Coleman's *EEO* data, also used school inputs aggregated to the school level. Hanushek (1970), Richard Murnane, and Donald Winkler used some pupil-specific data.

gregation.² Past studies have used an achievement level as the output measure;³ we use a value-added dependent variable—the change in achievement over a period of years. Finally, past studies have either relied on an additive form, or have segmented the sample, and estimated, conditional on *GSES*;⁴ we generalize these procedures by using interaction terms to allow the effect of policy variables (*TQ*, *SQ*, *PG*) to vary with *GSES*.

We conclude that empirical investigations have failed to find potent school effects because the aggregative nature of the data used disguised the school's true impact. The use of school and district averages introduced so much noise as proxies for inputs into students that effective inputs were not revealed. The use of achievement level as an output measure without controlling for the beginning level, clouded the examination of school inputs on that which schools can impact. And the lack of availability of pupil-specific data prevented an adequate exploration of the differential impacts of *TQ*, *SQ*, and *PG* on students with different *GSES*—and, in fact, in several instances have completely hidden the input effects.

Section I describes the sample and model estimation procedures. The major findings and caveats are in Section II. The importance of examining school inputs differentially is established in Section III by a

comparison of the results with and without interaction analysis. Section IV documents the importance of using disaggregated data by a comparison of the results when pupil-specific data for school inputs are used with the results when school averages are used. Some concluding remarks constitute Section V.

I. Description of Sample and Model Estimation Procedures

We had access to an exceptionally rich data base, a necessary condition to test the hypothesis that disgregation would reveal more significant inputs. This study is based upon data from 1970-71 pupil files for 627 sixth-grade elementary school students in 103 elementary schools. Schools were selected randomly from the Philadelphia School District, and students were randomly selected from their schools. Data were similarly gathered and analyzed for 553 eighth-grade students in 42 junior high, K-8, and middle schools, and for 716 twelfth-grade senior high school students in 5 senior high schools. Only the elementary school data and results are discussed in the body of this article.⁵ Analysis of the eighth-grade sample produced similar results—specific comparisons are noted in several footnotes. The twelfth-grade sample drew upon so few schools that the results were of limited interest.

A three-year personal educational history was compiled for each student. This was then matched with data on school-wide resources of the school the pupil attended, with the pupil's estimated family income.⁶

²In another study which analyzed the intradistrict distribution of school resources to disadvantaged students in Philadelphia, examination of detailed individual school data on each of twenty-six to thirty-one school inputs revealed useful policy insights. For example, while the instructional cost per pupil (a commonly used aggregated measure) was higher in schools with higher proportions of black and low income pupils, these schools were in general staffed by teachers with less experience and less education beyond the B.A. and by teachers who came from lower rated colleges and had scored lower on the National Teacher's Examination, Common. (See the authors, 1976.)

³All of the studies cited in fn. 1 used achievement as the dependent variable.

⁴Averch and Kiesling, Burkhead et al., Cohn, Katzman, and Levin used the additive form; Benson et al., Coleman (1966), Hanushek (1970, 1972), Kiesling, Michelson, and Winkler used subsamples; Murnane examined the interaction effect of achievement level.

⁵See the authors (1974) for the analysis and results of the eighth- and twelfth-grade samples, and for more detail on the sixth-grade sample.

⁶We have developed a procedure using 1970 Philadelphia Census data for estimating block income from block mean housing values, block mean contract rental values, tract distribution of block contract rental values, and tract distribution of income values. The block income appropriate to each pupil was taken to be his or her family income. This procedure involves 1) forming the cumulative distributions of data for each tract of owner-occupied housing values, contract rental values, and family income; 2) converting these cumulative distributions into relative distributions

and with data on each pupil's individual teachers. It was possible therefore to 1) look at pupils longitudinally, 2) examine a great many variables in a pupil-specific way (the teacher variables are of particular interest), and 3) go beyond simple linear specifications because of the fairly large number of observations.

The dependent variable chosen is the change in a composite achievement score—achieving—over the three-year period, third to sixth grades.⁷ Using the final score, which does not visibly control for initial achievement level, as the impact measure of resources is less satisfactory. This use of a value-added measure is consistent with the usual choice in estimating production functions. Further, the change formulation permits the prediction of the effect on pupil learning of educational input changes.⁸

(percentiles); 3) determining for each block the percentile in the tract distribution of mean owner-occupied housing value and the percentile for mean contract rental value; 4) determining the corresponding normal deviate arguments; 5) adjusting these by the regression coefficient for the tract between housing and income data for a cross-classified 20 percent sample; 6) assigning percentiles to the adjusted arguments; 7) finding the income values for these percentiles; 8) adjusting for differences in the income distribution of renters and owners; 9) averaging the two income values for each block. The procedure was carried out for black and nonblack income and housing distributions, and each pupil was assigned an income estimate on the basis of his or her race. See the authors (1975).

⁷The achievement score was measured in terms of grade equivalents—the norm for a sixth-grade student, tested in April of the sixth-grade year, would be 6.8, for example. For the students in the sample, the average sixth-grade score was 5.2. The grade equivalent form was preferable to either of the available alternative forms: the raw scores could not be compared over different grades, since the test differed, and the national percentile scores introduced changes in the nation's student population which were irrelevant.

⁸This formulation, it has been argued, is erroneous because the differences between initial and final score regress to the mean—that is, because tests have random error, there will tend to be a negative correlation between initial score and change in achievement. The concern is, of course, that if initial achievement is omitted from the right-hand side, the estimates of all the coefficients of variables correlated with initial achievement will be biased. However, when the regressions were run with and without the initial score, the

The independent variables were of three types: the genetic and socioeconomic character of the pupil (for example, family income, IQ, race), pupil-specific school inputs (for example, size of class of pupil's grade, qualities of the teachers who taught the pupil), and peer group characteristics (for example, proportion of high achievers, proportion of blacks). The relationship was examined using single equation multiple regression. Dummy variables, piece-wise linear fitting, and other nonlinearities were employed. Interactions of income, race, and/or achievement with school input and peer group variables were explicitly introduced.

Past attempts to estimate education production functions using a single equation regression method have been criticized on the grounds that they have ignored important problems of simultaneity. The argument, in particular, is that achievement gains affect student attitudes, and student attitudes affect achievement gains. Thus variables appearing on the right-hand side and treated as exogenous may in fact be endogenous. In this study, however, very few "psychological" variables are used. Only two of the variables might conceivably be regarded as problems. They measure the student's attendance record, and are included to reflect the student's motivation. However, reverse causality flowing from achievement (as measured by tests) to motivation seems to be tenuous: children are frequently unaware of their achievement test scores; grades and teacher attitudes are probably far more important in attitude reinforcement than Iowa scores; and there is considerable evidence that grades are poorly correlated with standard test scores. When the equations were estimated in the reduced form version, without the attendance variables, the coefficients were not changed in a way which altered any conclusions.⁹

variables which were significant remained so, and the changes in the coefficients were in no instance large enough to alter any of the broad conclusions drawn from the study.

⁹See the authors (1974, p. 54).

II. Summary of Findings

In winnowing down the original list of variables to get the equation of "best fit," many regressions have been run. A variety of functional forms—non-linear, interactive—for the variables have been examined. The data have been mined, of course. One starts with so few hypotheses convincingly turned up by theory that classical hypothesis testing is in this application sterile. The data are there to be looked at for what they can reveal.

The standard tests of significance provide guidance of only a very crude sort—hence, the usual asterisks are missing from the *t*-statistics in the tables. We threaded our way through the myriad of variable combinations with the magic 5 percent normal curve numbers as our North Star. But, to the final formulations and interpretations of coefficients, more informal standards were applied. All interaction results, for example, were checked out against separate regressions based upon data subsamples. Variables which had coefficients whose significance were very sensitive to the introduction and discarding of other variables were not retained. All the major findings were extensively tested for robustness to alternative specifications.

The coefficients and *t*-statistics¹⁰ for the equation of best fit for the sixth-grade sample are shown in column (2) of Table 1. The definitions, sources, $\bar{X}$'s, and σ 's of the variables are listed in the Appendix.

We are confident that the coefficients describe in a reasonable way the relationship between achieving and *GSES*, *TQ*, *SQ*, and *PG* for this collection of 627 elementary

school students in the Philadelphia School District. What we do have to be agnostic about is the absence of the results of replication.¹¹

A. Genetic and Socioeconomic Inputs

In addition to examining the effects of Income (row 1) and Race (row 2) on learning, interactions between school inputs and income and race of pupil were carefully examined—and a great deal of interaction was revealed. In fact, when these interactions are accounted for, no residual impact of Race and Income on achievement growth remained. The Sex (row 3) of the student affected learning—males did worse than females. Over the three-year period, males grew almost one month less in achievement scores than females.

The first-grade IQ (rows 4, 5, and 6) of the pupil had a strong effect on achievement growth. But an examination of the interaction of IQ with Race for high IQ students revealed that there was an additional positive effect for nonblack students only. That is, nonblack students with high IQ's grew more rapidly than those with lesser IQ's (2.8 months, compared with 1.3 months over the three years for each 10 point increment in the IQ score), but black students experienced similar achievement growths regardless of IQ. (It might be argued that, by controlling for initial ability, one channel through which *GSES* might affect achievement is eliminated. When the best fit equations were estimated without IQ, however, the coefficients related to Race and Income changed very little.)¹²

The motivation of students (proxied by Unexcused Absences and Latenesses) had a significant bearing on learning. Students with more Unexcused Absences (rows 7 and 8) and more Latenesses (rows 9 and 10)

¹⁰For interaction variables, the *t*-statistics in the table indicate only whether or not there is a significant difference in the impact of each input among different types of students. Tests of the form:

$$\hat{\theta} = \frac{\hat{\alpha} + \hat{\beta}y^*}{\sqrt{\hat{\sigma}_{\hat{\alpha}}^2 + \hat{\sigma}_{\hat{\beta}}^2 y^{*2} + 2\hat{\sigma}_{\hat{\alpha}\hat{\beta}}y^*}}$$

were used to determine the specific values of the interaction terms at which the results were significant. The interesting conclusion was that a considerable amount of variation over the range was revealed.

¹¹We are now in the midst of another phase of this study using a much larger sample of data from the same period (several thousand for each level of schooling). While it is regretful that these data are less pupil-specific, it enables the examination of the losses due to disaggregation in considerable detail.

¹²See the authors (1974, p. 51).

TABLE 1—REGRESSION RESULTS FOR SIXTH-GRADE SAMPLE OF PHILADELPHIA SCHOOL DISTRICT STUDENTS, 1968-71^c (*t*-values in parentheses)

Variables	Pupil-Specific School Inputs		School Averages of School Inputs	
	With Interaction Terms	Without Interaction Terms	With Interaction Terms ^a	Without Interaction Terms
(1)	(2)	(3)	(4)	(5)
1. Income	.06 (.34)	.17 (1.08)	-.002 (-.009)	.21 (1.26)
2. Race	-.23 (-.10)	-3.34 (-2.58)	.74 (.31)	-3.03 (-2.47)
3. Sex	-.90 (-1.39)	-1.23 (-1.88)	-.83 (-1.25)	-.96 (-1.45)
4. IQ	.13 (3.64)	.13 (3.75)	.15 (4.02)	.15 (4.31)
5. IQ 110+	.15 (1.40)	.09 (.98)	.14 (1.32)	.05 (.58)
6. (5) x Race	-.22 (-1.78)	-	-.22 (-1.75)	-
7. Unexcused Absences	.14 (.53)	-.30 (-3.63)	.13 (.47)	-.26 (-3.17)
8. (7) x Income	-0.6 (-1.84)	-	-.05 (-1.54)	-
9. Latenesses	-1.11 (-3.38)	-.20 (-2.00)	-1.20 (-3.45)	-.25 (-2.36)
10. (9) x Income	.11 (2.93)	-	.11 (2.82)	-
11. Rating of Teacher's College, 1 ≥ 525	13.57 (2.45)	4.03 (2.57)	-2.92 (-.55) ^b	-3.35 (-1.68)
12. (11) x Income	-.99 (-1.74)	-	.04 (.08)	-
13. Teacher's Experience	-.47 (-1.80)	.07 (.73)	-.70 (-1.06)	-.05 (-.11)
14. (13) x Third-Grade Score	.02 (2.29)	-	.02 (1.22)	-
15. National Teacher Exam Score	-.02 (-2.62)	-.02 (-2.83)	-.005 (-2.4)	-.02 (-1.04)
16. Class Size ≥ 33	-.16 (-.10)	-.82 (-.54)	.13 (.08)	.20 (.13)
17. Class Size, 28-33	-4.22 (-1.74)	-.20 (-.14)	-1.32 (-.52)	.17 (.17)
18. (17) x Third-Grade Score	.14 (2.22)	-	.06 (.79)	-
19. School Enrollment	-.002 (-1.54)	-.003 (-2.33)	-.001 (-1.33)	-.003 (-1.80)
20. (19) x Race	-.004 (-1.65)	-	-.005 (-1.84)	-
21. Library Books/Pupil	-.43 (-1.97)	-.47 (-2.21)	-.12 (-.70)	-.13 (-.81)
22. Percent Blacks ≥ 20 and < 40	3.70 (2.53)	3.08 (2.15)	3.99 (2.49)	3.81 (2.47)
23. Percent Blacks ≥ 40 and < 60	6.02 (3.61)	5.64 (3.34)	4.02 (2.37)	3.55 (2.13)
24. Percent Blacks ≥ 60	4.18 (2.62)	3.10 (1.93)	3.95 (2.34)	2.74 (1.69)
25. Percent High Achievers	.68 (2.55)	.15 (1.60)	.69 (2.05)	.17 (1.75)
26. (25) x Third-Grade Score	-.01 (-2.20)	-	-.01 (-1.73)	-
27. Percent Low Achievers	-.08 (-2.11)	-.08 (-2.28)	-.07 (-1.52)	-.08 (-1.85)
28. Disruptive Incidents	1.86 (3.89)	.35 (2.09)	1.50 (2.69)	.37 (2.23)
29. (28) x Third-Grade Score	-.05 (-3.18)	-	-.03 (-2.00)	-
Constant	22.09	25.09	11.70	21.82
R ²	.27	.24	.23	.21
F	9.03	10.68	7.50	9.52

Note: Dependent Variable: Sixth-Grade Score on Iowa Test of Basic Skills (Composite) Minus Third-Grade Score.

^aBecause they are less pupil-specific, variables 11 through 29 are not identical in columns (4) and (5) to those in columns (2) and (3). Exact definitions for all the variables are in the Appendix.

^bWhen school averages were tested, it was no longer possible to use the best fit breaking point of the 525 undergraduate college rating, because too few averaged observations were at or above 525. A rating of 450 was used, therefore. In column (2) the intercept term had a coefficient of 4.23 and a *t*-statistic of 1.82; the interaction term had a coefficient of -.49 and a *t*-statistic of -1.95. In column (3), the term reads -.10 (-1.4).

^cSources of data are listed in the Appendix.

grew less. Interestingly, it was the middle and higher income students whose growth in scores registered the greatest declines when their Unexcused Absences were greater—perhaps, because when an advantaged student misses school it signifies a far more serious negative attitude toward schooling than when a disadvantaged student does the same thing. (Five more unexcused absences were associated with 2.1 months less growth over three years for students with a \$10,000 family income, and only .7 months less for those with a \$5,000 income.) In the case of Latenesses, however, it was only the lower income students where the effect was pronounced.

Other socioeconomic factors examined, but not found to be significant, were the number of residential moves of the students, whether or not the pupil was foreign born, and whether or not both parents were foreign born.

B. School Inputs: Teacher Quality¹³

Finding out which school inputs are helpful to learning is what we are, of course, most interested in. These are the things that school administrations and teachers' unions can, if the spirit is willing, do something about.

The unique detail of the data base on the characteristics of teachers makes possible a deeper inquiry into the question "what matters?" than is possible with noisy, aggregative data. Several "quality" measures of each of the teachers were obtained and the specific teachers each pupil had were identi-

fied. What mattered?¹⁴ Teachers who received B.A.'s from higher rated colleges¹⁵ (rows 11 and 12) were associated with students whose learning rate was greater—and it was students from lower income families who benefitted most. (A student whose family income was \$5,000 grew 8.6 months more with a teacher from a higher rated college than with teachers from other colleges; a student whose family income was \$10,000 grew 3.7 months more.) Teacher experience (rows 13 and 14) has been found to be unimportant in many studies.¹⁶ Though it seems clearly unreasonable to expect equal effectiveness of experience for all levels of student abilities, the interaction between them has not been fully examined. We found that students whose third-grade score was above grade level benefitted from more experience, but those who were very much below grade level were negatively affected. In fact, this latter group did best with newer teachers who perhaps have an undampened enthusiasm for teaching those who find it hard to learn. These teacher quality findings were robust. They remained essentially intact whether all or some of the teacher quality variables were controlled, and when the sample was broken into subsamples by

¹³The qualities of sixth-grade teachers, rather than fourth-, fifth-, and sixth-grade teachers, were used. Evidence that the sixth-grade teachers had the strongest impact piled up: The R^2 for the equation using sixth-grade teachers was significantly higher than using either of the other years. The Koyck lag formulation (see Zvi Griliches) which assumes that the weights assigned diminish geometrically for more distant periods was used. The R^2 's, coefficients, and t -statistics for the college rating and experience of teachers, for the equation which gives most weight to the sixth-grade teacher qualities, and least to the fourth and fifth, are significantly higher than in the alternate specifications. They diminish continuously as the weight assigned to the sixth-grade teacher decreases.

¹⁴Hanushek (1970) and Murnane both had some teacher data which were specific to the pupil. Both used dummy variables for the teachers, testing to find out whether or not teachers mattered. For his subsample of Anglo-American students, Hanushek showed the quality of teachers mattered, but not for the subsample of Mexican-Americans (perhaps, he hypothesized, because of the language problem). Murnane concluded that teaching quality did matter. While the use of teacher dummies did not demonstrate that teacher quality can be proxied by any objectively measurable quality, it does give support to the conclusion that the use of pupil-specific teacher variables results in more revealed impact on achievement.

¹⁵Winkler, in California, also found that graduates of "good" colleges taught significantly better than graduates of "bad" colleges.

¹⁶Coleman found that teacher experience mattered only marginally. Hanushek found no statistical significance to teacher experience in any of his three California subsamples. Murnane found that having experience of one, two, or three years over having none mattered greatly; the extra benefit of between three and five years was less; and, beyond five years, he found no effect of experience on achievement.

TABLE 2—RESULTS ON ACHIEVEMENT OF TEACHER'S COLLEGE RATING AND EXPERIENCE USING SUBSAMPLES

	Low Income ^a	Middle Income ^a	High Income ^a
Rating of Teacher's College, $I \geq 525^b$	15.32 (4.16)	.09 (.03)	3.49 (1.64)
	Low Achievers ^c	Middle Achievers ^c	High Achievers ^c
Teacher's Experience ^d	-.01 (-.04)	.16 (.91)	.31 (1.64)

^aLow income is defined as < \$7000, middle income, as $\geq$ \$7000 and $\leq$ \$9000, and high income as > \$9000.

^bThe comparable coefficients and tests of significance, using interaction terms, were 8.62 (2.95) for students whose family income was \$5000, 5.64 (3.28) for those at an \$8000 income level, and -1.29 (-0.36) for those at a \$15,000 level.

^cLow achievers are those whose third-grade Iowa Test scores were in grade equivalent terms $\leq$ 2.7; middle achievers scored > 2.7 and < 3.9; high achievers scored $\geq$ 3.9.

^dThe comparable coefficients and tests of significance, using interaction terms, were -.29 (-1.54) for students who scored two grades below level (at 1.0), .05 (0.53) for those who were at grade level (3.0), and .39 (2.42) for those who were two grades above (at 5.0).

race, achievement, and income. The relevant coefficients and *t*-statistics for the subsamples are shown in Table 2.

Finally, we found a perverse relationship between the National Teacher Exam Score (row 15) and learning. The discriminatory powers of the exam were evaluated by the Philadelphia School District in 1972. They concluded that the scores should not be used as the only measure of the potential of a teacher—these findings suggest that they should not be used as any measure.

C. School Inputs: Nonteacher School Quality

Does Class Size (rows 16, 17, and 18) matter? Educators' studies cover the full range of answers. One summary report (see Howard Blake) stated that thirty-five of the studies surveyed found that smaller classes were more effective, eighteen found that larger classes were more effective, and thirty-two were inconclusive! More light is shed, however, when the differential effects of smaller and larger classes are examined in relation to achievement levels (rows 16, 17, 18). Low-achieving students (those scoring two or more years below grade level) did worse (almost three months worse over the three years) in classes with more than 28 students; high-achieving students (those scoring two or more grades above grade

level) did better (almost three months better); those around grade level appeared to be unaffected. When the Class Size variable was explored through ability subsamples, rather than interactions, the same results emerged. The relevant coefficients and *t*-statistics are shown in Table 3.

School Enrollment (rows 19 and 20), also a variable about which studies have produced a wide range of possible impact, is shown in this study to have differential effects on black and nonblack students. Smaller schools appeared to be better for all, but had a larger beneficial effect on achievement growth for black pupils. Library Books per pupil (row 21) had a perverse effect—possibly because the fact that libraries were larger in relation to the number of pupils in schools where there were more low-income pupils was not adequately picked up by the other variables.¹⁷

There appear then to be a number of school inputs that can raise student achievement. Equally interesting was the discovery that a number of inputs which we pay for did not accomplish this goal. 1) The general physical facilities of schools did not seem to make much difference, one way or another, to students' learning. Whether the pupil

¹⁷Coleman stated that "The number of volumes per student in the school library shows small and inconsistent relations to achievement" (p. 316).

TABLE 3—RESULTS FOR CLASS SIZE USING ABILITY-GROUPED SUBSAMPLES^a

	Low Achievers	Middle Achievers	High Achievers
Class Size $\geq 33^b$	-2.75 (-1.22)	1.48 (.58)	3.00 (.75)
Class Size, 28-33 ^b	-4.27 (-1.98)	2.12 (.89)	5.42 (1.43)

^aFor definitions of subsamples, see fn. c, Table 2.

^bThe comparable coefficients and tests of significance, using interaction terms, were -2.83 (-1.44) for students who scored two grades below level (at 1.0), -.05 (-.04) for those who were at grade level (3.0), and 2.72 (1.48) for those who were two grades above (at 5.0).

had access to more or less playground space, a new or old building, or a building rated higher or lower in general physical condition, did not seem to matter much when it comes to achievement test scores. 2) There was a wide range among Philadelphia elementary school principals in experience, extra degrees, and extra educational credits—but there were no discernible differences in the impact on achievement. 3) Whether teachers have more or less education beyond the B.A. did not seem to make his or her students learn more. The finding of an absence of impact on achievement of extra training is consistent with many studies¹⁸—yet teacher salary scales nearly everywhere incorporate substantial rewards to teachers who take additional educational credit beyond the B.A.

Clearly, though these inputs do not seem to relate to achievement growth, it does not mean that reducing these expenditures to zero is the logical policy recommendation. Without some minimum level, there might be some negative effect, and with much more than is now put in, there might be some positive effect. Further, and a point not to be ignored, the objective of many school inputs is not limited to the one used here—growth in achievement scores. Other objectives—attitudes toward other races, sense of participation in the democratic process—may be part of the desired outcome.

¹⁸Coleman, Burkhead et al., Hanushek, Kiestling, and Winkler all found that having or not having an M.A. was not significantly related to achievement. The literature in education journals on this subject reveals results on both sides.

D. Peer Group Effects

School segregation, in the sense of heavily concentrated racial mixtures, exists in the Philadelphia schools. In the years covered by the study sample, 23 percent of the elementary schools had less than 10 percent black students, and 40 percent had more than 90 percent. Busing for desegregation did not exist, and could obviously introduce entirely new elements not reflected in our findings. We found that black and non-black students benefitted (rows 22, 23, and 24)—had the largest growth in achievement—when they were in schools with a 40 to 60 Percent Black student body, rather than in schools that were more racially segregated.¹⁹ From a policy point of view, of course, what one is interested in is the effect of changes in the proportion of black students when variables associated with these changes are not controlled. The finding that the largest growth in achievement took place with a 40 to 60 Percent Black student body remained intact, when the variables measuring the Proportion of High

¹⁹One is tempted to dissect this somewhat unexpected (for nonblacks) result by searching for characteristics of the families that live in racially mixed areas that would contribute to learning growth, but which are not captured by the variables in the equation. In addition to the family income variable already in, variables measuring the average income of residents in the school feeder area, the change in relative income of the feeder area between the 1960 and 1970 Censuses, and the average education level of the 25-year and over population were tried and found to be entirely without significance. In addition, dummy variables for the two areas of Philadelphia which have had the most stable pattern of racial integration were explored and found to be not significant.

TABLE 4—RESULTS ON ACHIEVEMENT GAIN OF THE PROPORTION OF BLACK STUDENTS WITH AND WITHOUT CONTROLLING FOR ASSOCIATED VARIABLES

Independent Variable	Controlling for Variables 25, 26, 27, 28, 29	Without Controlling for Variables 25, 26, 27, 28, 29
22. Percent Blacks ≥ 20 and < 40	3.70 (2.53)	2.45 (1.73)
23. Percent Blacks ≥ 40 and < 60	6.02 (3.61)	4.25 (2.76)
24. Percent Blacks ≥ 60	4.18 (2.62)	2.80 (2.04)

Achievers (rows 25 and 26), the Proportion of Low Achievers (row 27), and the number of Disruptive Incidents (rows 28 and 29) were not included. (See Table 4.) The coefficients and *t*-statistics were slightly lower.

Ability grouping can be regarded as another type of segregation. What are the effects of being in a grade with more or less very high or very low achievers (rows 25, 26, and 27)? Seventy percent of the elementary schools had less than 10 percent high achievers; 52 percent had more than 50 percent of the student body achieving at very low levels. In elementary schools, students who test at grade level or lower are distinctly helped by being in a school with more high-achieving students. The students who are performing above their grade level are not particularly affected—it is the low-achieving student: who benefit most. This finding is so, whether or not the proportion of black students is controlled. Being in a student body with more low achievers has a negative effect on learning for all students, though this is not so if there is no control for racial composition. What all this seems to say is that high achievers are relatively unaffected by variations in the percentage of top achievers. But, for the low achievers, the intellectual composition associated with other characteristics of the student body has a direct impact on learning.

Finally, what is one to say about the finding that, for students who are at or below grade level, more Disruptive Incidents (rows 28 and 29) are associated with greater achievement growth? It is only for those well above grade level that we found the expected negative effect. This may well be a case where very different results would be

obtained if the data were available on a classroom-specific basis. If disruptive incidents only broke out in classrooms where achievement growth barely occurred, then spreading the impact among all classes may have produced this anomalous effect. In any case, it would seem a bit premature to engage in a policy of encouraging disruptive incidents to increase learning!

Again, it is interesting to note a number of peer group characteristics that were not significant in any of the specifications: percent of the student body receiving free lunches, student mobility in the school, median income of the school feeder area, average education level of adults, (25 years and over) in the school feeder area, change in the relative income of the area from 1960 to 1970, and average daily attendance in the school.

In summary, then, we find that many school inputs mattered, and that their impact varied considerably on different types of students. There were inputs that helped disadvantaged students. Clearly, this is consistent with what educators and parents believe when they advocate individualizing education.

IV. What Was Revealed By Interaction Analysis of Inputs?

On the basis of this study, at least, it would appear that there is much to be gained from examining the differential impact (by race, income, and achievement level) of educational inputs on achievement growth. How much there is to be gained can be seen more precisely by a comparison of columns (2) and (3) in Table 1. In column (3), the results that would have been

produced by this pupil-specific data base, if interactions had not been analyzed, are given. Apart from the reduced explanatory power of the equation, three general types of insight emerge.

First, the expected negative effects of low income and race have been so extensively traced through specific inputs in column (2) that the Income and Race intercepts have no significance there at all. But when they are not traced through, in column (3), the coefficient of the Income variable becomes larger and the Race variable acquires a very large and significant negative coefficient.²⁰ This is, of course, consistent with the important and repeated finding of many studies searching for the determinants of student achievement. But, the lack of insight into just where—in connection with which inputs—Income and Race play a role, has left schools relatively impotent in changing the achievement growth of students.

Second, a possible explanation for the hopelessness of the findings of many studies is that the effectiveness of some inputs may be of directly opposite direction for different types of students. We found this in the case of three variables. Without interaction analysis, the conclusions would have been that there was no additional positive effect of having an IQ of 110 or more, that the experience of teachers had no effect on achievement growth, and that class size was irrelevant to learning (rows 5, 13, 16, and 17, column (3)). Interaction analysis revealed that the nonblack student with an IQ of 110 or more moved much more rapidly ahead, but the black student did not; that higher achieving students benefitted by being with more experienced teachers,²¹ but lower achieving students were adversely affected—in “fact,” benefitting from newer teachers; and that low achievers did better in classes with less than 28 students than in

classes with 28 to 33, while high achievers did worse.²²

Finally, without examining the interactive effects of inputs, the policy implications of important differential effects (not necessarily of opposite sign) are lost. From column (3) it would not be possible to “know” that higher income students were most negatively affected by more Unexcused Absences,²³ that only the low-income students were adversely affected by more latenesses, that the lower income students benefitted most (and considerably) by having teachers who came from higher rated colleges, that black students experienced the bigger boost to learning from being in smaller schools (School Enrollment), that the low achiever is the real beneficiary of being in a grade with higher Percent High Achievers (the high achiever is unaffected), and that the expected negative effect of being in a school with more serious Disruptive Incidents shows up only for the very high achievers.

V. What Was Revealed By Pupil-Specific Data on School Inputs?

It would appear from comparing the results using pupil-specific data on inputs (Table 1, column (2)) with the results using school averages of inputs (column (4)) that there is much to be learned from the use of the more disaggregated data base. Many studies have been hampered by the limited amount of data available (easily obtainable?) which are specifically tied to the pupil. Thus, in Coleman's seminal work, and in the reanalysis of his *EEO* data, the only data used bearing on the influence of teacher experience were the average experience levels in schools—not, as we were able to obtain for this study of Philadelphia

²⁰Results, similar in direction, showed up in the eighth-grade sample.

²¹In the eighth-grade sample, the experience of the social studies teacher lost significance when interactions were not explored.

²²The negative impact of being in a class below thirty-two was reduced, and the very large negative effect for low income students of being in classes larger than thirty-two was lost, when interactions were not used in the eighth-grade sample.

²³In the eighth-grade sample, it would not have been possible to “know” that average and above average achievers were the only ones negatively affected.

schools, the experience level of the specific teachers confronting each pupil.

The most interesting results relate to the totally changed findings on the teacher variables. The rating of their undergraduate colleges (Table 1, rows 11 and 12), their experience (rows 13 and 14), and their teacher exam score (row 15) were not significant when averages were used. And, equally interesting, the class size (rows 16, 17, and 18), showed no impact when it was measured in a way not specific to the pupil.²⁴

Other school input and climate variables which, in column (2), were changed over the three years the student was tracked (so that if the student changed schools, a change in the school variable was incorporated) were reduced in significance, when the tracking was not done. Thus, when just the sixth-grade value of the variable was used, the impact of the number of library books (row 21), of the size of the school on nonblacks, (rows 19 and 20), of the proportions of high- and low-achievers (rows 25, 26, and 27), and the proportions of black students (rows 22, 23, and 24) in the school were reduced.

VI. Concluding Remarks

The findings of this study suggest 1) that when there are extensive pupil-specific data available, more impact from school inputs is revealed; 2) that when the effects of school inputs are examined differentially, more impact and insight emerge; 3) that most of the effects of family income and race can be tagged to specific school inputs; and 4) that the low achiever, the low-income student, and the black student do respond in terms of achievement growth to some school inputs.

²⁴In the eighth-grade sample, when the same comparison was made between the results using pupil-specific data and not, similar findings emerged: the impact of the undergraduate college rating for social studies teachers became weaker, the experience of English teachers lost significance, the experience of math teachers was reduced in significance, and class size had no significance.

A comparison of the results in column (5) (where school-wide averages of school inputs were the data and no interaction analysis was used) with the results in column (2) (where the data on school inputs were specific to the pupil and differential impacts were examined) should encourage the pursuit of disaggregated data bases. No impact from teacher experience or the exam score of the teacher emerges (rows 13 and 15). The rating of the teacher's college has a perverse effect (row 11). Class size has no effect (rows 16, 17, and 18). And the impact of the proportion of blacks (rows 22, 23, and 24), the proportion of high achievers (rows 25 and 26), and the proportion of low achievers (row 27) is reduced.

Perhaps most interesting from the point of view of those seeking to make more equal the achievement growth of the advantaged and the disadvantaged, is the finding that there appear to be school inputs which help the disadvantaged do better. If the courts are to rule on these matters, then targeting which school resources are specifically helpful to the disadvantaged is essential. If, for example, physical facilities are not particularly helpful to achievement, then learning that schools in better condition tend to be in locations where there are fewer disadvantaged students should not necessarily lead to action to equalize the facilities. If, on the other hand, the fact that smaller classes are particularly helpful to the learning growth of low achievers is verified, then there is something specific to advocate in the quest for equity. The findings of this study suggest that there are school inputs which help the low achievers to do better. And most of these were findings which emerged only when pupil-specific data were used and examined in relation to student characteristics.

APPENDIX

The definitions, $\bar{X}$'s, and σ 's of each of the variables shown in Table I are listed here with the corresponding row numbers. The prime notation is used for the variables

in columns (4) and (5) which correspond to the variables in columns (2) and (3).

1. Income: Estimated family income (in thousands); $\bar{X} = \$8.802$; $\sigma = \$2.609$.
2. Race: Dummy variable, 0 = nonblack, 1 = black; $\bar{X} = .64$; $\sigma = .48$.
3. Sex: Dummy variable, 0 = female, 1 = male; $\bar{X} = .48$; $\sigma = .50$.
4. IQ: Score on first-grade Philadelphia Verbal Ability Test; $\bar{X} = 100.65$; $\sigma = 14.46$.
5. IQ 110+: Additional effect of scoring 110 or more on IQ test; form is a two-piece linear function with corner at 110.
6. (5) x Race: Interaction of Race with IQ 110+.
7. Unexcused Absences: Average of annual number of unexcused absences over three years, 1967/68-1970/71; $\bar{X} = 2.33$; $\sigma = 4.48$.
8. (7) x Income: Interaction of Income with Unexcused Absences.
9. Latenesses: Average of annual number of latenesses over three years, 1967/68-1970/71; $\bar{X} = 2.02$; $\sigma = 3.59$.
10. (9) x Income: Interaction of Income with latenesses.
11. Rating of Teacher's College, $1 \geq 525$: Gourman rating of sixth-grade teacher's undergraduate college, $0 = < 525$, $1 = \geq 525$; $\bar{X} = .05$; $\sigma = .22$.
- 11'. School $\bar{X}$ of Ratings of Teacher's Colleges, $1 \geq 450$: School average of Gourman ratings of all teachers' undergraduate colleges, $0 = < 450$, $1 = \geq 450$; $\bar{X} = .04$; $\sigma = .21$.
12. (11) x Income: Interaction of Income with Rating of Teacher's College, $1 \geq 525$.
- 12'. (11') x Income: Interaction of Income with School $\bar{X}$ of Ratings of Teacher's Colleges, $1 \geq 450$.
13. Teacher's Experience: Sixth-grade teacher's experience (in years up to 11); $\bar{X} = 6.58$; $\sigma = 3.71$.
13. School $\bar{X}$ of Teacher's Experience: School average of all teachers' experience (in years up to 11); $\bar{X} = 7.28$; $\sigma = 1.08$.
14. (13) x Third-Grade Score: Interaction of Third-Grade Score with Teacher's Experience.
- 14'. (13') x Third-Grade Score: Interaction of Third-Grade Score with School $\bar{X}$ of Teacher's Experience.
15. National Teacher Exam Score: Sixth-grade teacher's score on National Teacher Examination, Common; $\bar{X} = 606.83$; $\sigma = 58.93$.
- 15'. School $\bar{X}$ of Teacher's National Teacher Exam Scores: School average of all teachers' National Teacher Examination, Common, scores; $\bar{X} = 606.91$; $\sigma = 19.94$.
16. Class Size ≥ 33 : Dummy variable, $1 = \geq 33$, $0 = < 33$; class size is the average size of the class in pupil's grade in pupil's school, $\bar{X} = .30$; $\sigma = .46$.
- 16'. School $\bar{X}$ of Class Size ≥ 33 : Dummy variable, $1 = \geq 33$, $0 = < 33$; class size is average of all classes in school; $\bar{X} = .17$; $\sigma = .38$.
17. Class Size, 28-33: Dummy variable, $1 = \geq 28$ and < 33 , $0 = < 28$ or ≥ 33 ; class size is the average size of the class in pupil's grade in pupil's school; $\bar{X} = .63$; $\sigma = .48$.
- 17'. School $\bar{X}$ of Class Size, 28-33: Dummy variable, $1 = \geq 28$ and < 33 , $0 = < 28$ or ≥ 33 ; class size is average of all classes in school; $\bar{X} = .66$; $\sigma = .48$.
18. (17) x Third-Grade Score: Interaction of Third-Grade Score with Class Size, 28-33.
- 18'. (17') x Third-Grade Score: Interaction of Third-Grade Score with School $\bar{X}$ of Class Size, 28-33.
19. School Enrollment ($\bar{X}$): Average of number of pupils enrolled in each school pupil attended, 1967/68-1970/71; $\bar{X} = 917.35$; $\sigma = 336.05$.
- 19'. School Enrollment (6): Number of pupils enrolled in school pupil attended in sixth grade; $\bar{X} = 912.90$; $\sigma = 343.79$.
20. (19) x Race: Interaction of Race with School Enrollment ($\bar{X}$).
- 20'. (19') x Race: Interaction of Race with School Enrollment (6).

21. Library Books/Pupil ($\bar{X}$): Average number of library books per pupil in each school pupil attended, 1967/68-1970/71; $\bar{X} = 6.98$; $\sigma = 2.37$.
- 21'. Library Books/Pupil (6): Number of library books per pupil in school pupil attended in sixth grade; $\bar{X} = 7.88$; $\sigma = 3.00$.
22. Percent Blacks ≥ 20 and < 40 ($\bar{X}$): Dummy variable: 1 = Percent blacks in school $\geq 20\%$, and $< 40\%$; average of each school pupil attended, 1967/68-1970/71; $\bar{X} = .09$; $\sigma = .29$.
- 22'. Percent Blacks ≥ 20 and < 40 (6): Dummy variable described in 22, for sixth-grade school; $\bar{X} = .08$; $\sigma = .27$.
23. Percent Blacks ≥ 40 and < 60 ($\bar{X}$): Dummy variable: 1 = Percent blacks in school $\geq 40\%$, and $< 60\%$; average of each school pupil attended, 1967/68-1970/71; $\bar{X} = .10$; $\sigma = .31$.
- 23'. Percent Blacks ≥ 40 and < 60 (6): Dummy variable described in 23, for sixth-grade school; $\bar{X} = .11$; $\sigma = .32$.
24. Percent Blacks ≥ 60 ($\bar{X}$): Dummy variable: 1 = Percent blacks in school $\geq 60\%$; average of each school pupil attended, 1967/68-1970/71, $\bar{X} = .52$; $\sigma = .50$.
- 24'. Percent Blacks ≥ 60 (6): Dummy variable described in 24, for sixth-grade school; $\bar{X} = .51$; $\sigma = .50$.
25. Percent High Achievers: Average percent in pupil's fifth and sixth grades scoring above 84th National Percentile on Iowa Test of Basic Skills; $\bar{X} = 4.00$; $\sigma = 5.93$.
- 25'. Percent High Achievers in School: Percent of pupils in all grades of pupil's sixth-grade school scoring above 84th National Percentile on Iowa Test of Basic Skills; $\bar{X} = 5.56$, $\sigma = 6.55$.
26. (25) x Third-Grade Score: Interaction of Third-Grade Score with Percent High Achievers.
- 26'. (25') x Third-Grade Score: Interaction of Third-Grade Score with Percent High Achievers in School.
27. Percent Low Achievers: Average Percent in pupil's fifth and sixth grades

scoring below 16th National Percentile on Iowa Test of Basic Skills; $\bar{X} = 48.75$; $\sigma = 18.50$.

- 27'. Percent Low Achievers in School: Percent of pupils in all grades of pupil's sixth-grade school scoring below 16th National Percentile on Iowa Test of Basic Skills; $\bar{X} = 45.41$; $\sigma = 17.39$.
28. Disruptive Incidents ($\bar{X}$): Average of annual number of disruptive incidents in each school pupil attended, 1967/68-1970/71; $\bar{X} = 2.89$; $\sigma = 2.31$.
- 28'. Disruptive Incidents (6): Number of disruptive incidents in school and year pupil attended in sixth grade; $\bar{X} = 2.85$; $\sigma = 2.39$.
29. (28) x Third-Grade Score: Interaction of Third Grade Score with Disruptive Incidents ($\bar{X}$).
- 29'. (28') x Third-Grade Score: Interaction of Third Grade Score with Disruptive Incidents (6).

Sources: 1: The authors (1975), Individual Pupil Records, Form EH-7, and School District of Philadelphia (SDP) 1960-70 Tape on Pupil Addresses. 2-10: Individual Pupil Records, Form EH-7 and Roll Sheets. 11-12': Jack Gourman and SDP Permis File. 13-15': SDP Permis File. 16-20': Monthly Reports, Field Operations and Subsidies Division, SDP. 21-21': Office of Research and Evaluation, SDP. 22-24': *Enrollment, Negro and Spanish-Speaking in the Philadelphia Public Schools*. 25-27: *Reports of Division of Testing*, Office of Research and Evaluation, SDP. 25'-27': Office of Research and Evaluation, *Reports of Spring Achievement Test Results*. 29-29': Facilities Security, SDP. *Reports on Incidents by School Code, Date, and Incident Classifications*.

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